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A REPLY TO PROFESSOR SAFFORD

By F. M. URBAN, University of Pennsylvania

Professor Safford published in the January number of this *Journal* a little note containing a criticism of my theory of psychophysical measurements. His objections are two in number. The first refers to the number of decimals which have been retained in my tables; he is of the opinion that the computation should have been carried to the third decimal only rather than to the fourth. The second objection is of a more complicated nature, and refers to my use of Lagrange's formula. Professor Safford's ideas are very interesting and I am glad to have an opportunity to explain some considerations, at which my original articles merely hint. My book would have become very voluminous, had I undertaken to present all the chains of reasoning which I later found to be wrong, or to describe all the considerations which decided me to adopt a certain manner of procedure. Both of Professor Safford's objections occurred to me in the course of working out my data, and I may be allowed to state the reasons why I believe that they are erroneous.

Before answering the first objection I want to say that in computing data of this sort it is customary to express one's results in four places of decimals. The majority of statistical investigations, however, do not use relative frequencies, as I did, but percentages calculated to the second decimal. Percentages are found from relative frequencies by multiplying by 100; two decimals in percentages, therefore, correspond to four decimals in relative frequencies. Saying that an event has the relative frequency 0.4422 is the same as saying that this event occurs in 44.22 per cent. of the cases. The latter form is, perhaps a little more familiar to the eye; but relative frequencies have the advantage over percentages of being the more primitive notions.

The relative frequencies of the different judgments form the starting-point of my exposition of the theory of psychophysical measurements; and I naturally gave much thought to the question of how many decimals should be retained. The mere physical labor of carrying out the computations—including the unavoidable wild goose chases—was very considerable indeed, and it seemed highly desirable not to increase the task by carrying too many decimals. Not being satisfied with the reference to custom and being acquainted with the theory of physical measurements, my mind naturally drifted into the channels pointed out by Professor Safford. I found out very soon that there does not exist a universal agreement as to the number of decimals which should be retained in the result, and that the rules explained by Professor Safford are not the only way of approaching the problem, for no less an authority than Gauss advocates the rule that the computations should be carried so far that the final result should enable one to calculate the actual data of observation with their original precision.

Professor Safford says that it is customary to use the average deviation as a measure of precision and to retain two significant figures of it. There does not exist any such general custom. In Germany and Austria the mean error is in almost exclusive use, while English, American and some French textbooks on the method of least squares recommend the probable error. Something may be said in favor of each one of these quantities, but this is not the

topic of the present discussion, where we only want to see whether there exists a universal agreement as to the quantity which is to be used as a measure of precision. Neither does there exist an agreement as to the number of significant figures to be retained in the measure of precision. I open the chapter on the adjustment of observations in Czuber's text-book of the calculus of probabilities and find on pages 293, 294 and 298 examples in which the mean error is calculated to one, two, three and four significant figures. Some of these examples are taken from authoritative sources, so that one cannot possibly say that there exists a general rule as to the number of decimals which should be retained in the result. Two or three significant figures in the measure of precision seem to be most frequently used.

There exists a fundamental difference between the data of statistical observation and the results of physical measurements, which Professor Safford entirely overlooks. The results of physical measurements are exact within one-half of the last significant figure. Thus if we put down 29 inches as the length of a line, this result means that the line is not longer than 29.5 nor shorter than 28.5 inches. When using this result in a computation, one must not add digits to it, because the following figures are entirely unknown. The case of statistical observation is different. If we observe that an event takes place 29 times in 100 cases, both these numbers are absolutely exact. The figure 29 does not indicate a result which may vary between 28.5 and 29.5; but it means exactly 29 and we may add as many zeros as seems necessary. The number of decimal places retained is merely a question of convenience, and one cannot be accused of publishing a misleading result if the accuracy of the determination accompanies it.

It took me some time to see that the theory of physical measurement is not the most direct way of determining the precision of my observations. The original data of my experiments are determinations of the probabilities of the different judgments. The most direct way of finding the precision of these observations is given by Bernoulli's theorem. This theorem refers to observations, in which a chance event A occurred n times in a total number of cases N ; and it gives the most probable value of the unknown probability of this event and the limits of the accuracy of this determination. This is exactly the case of my experiments and I chose the probable error determined by Bernoulli's theorem as the measure of precision. A table of these probable errors is printed in the *Archiv f. d. ges. Psychologie*, 1909, Vol. 15, p. 287. The probable errors in the determination of the probabilities of the "greater" judgments for Subject I on the comparison stimuli 84, 88, 92, 96, 100, 104 and 108 were found to be 0.0015, 0.0044, 0.0090, 0.0132, 0.0157, 0.0097 and 0.0076. Admitting that two or three significant figures in the measure of precision is a conservative accuracy, I decided to retain four decimals in the tables of the relative frequencies of the judgments.

It was the traditional custom in statistics and psychophysics to use the methods of physical measurement uncritically. Lexis and his followers have shown how statistical data must be treated, and I tried to develop the theory of psychophysical measurement. That this can be done has been shown. At present we have a number of psychophysical methods which may stand alone on their own merit. Neither did I decide hastily, in breaking away from the old notions. It may be that there is a connection between the theories of physical and of psychophysical measurement; but at present we know only little about it, and the only statement which one could make with any kind of confidence is, that the theory of physical measurement must be based on that of psychophysical measurement. The so-called law of the distribution of errors of observation has resisted all attempts at a purely mathematical demonstration. Innumerable attempts have been made to explain this law—some of them by the cleverest mathematicians the world has known—but all have failed in so far as their proofs necessitated the introduction of some one assumption which is equivalent to the propo-

sition to be proved. It is only natural to suppose that this law contains some supposition of non-mathematical nature. It seems that it depends, in some way, on our method of making observations, and that the so-called Gaussian coefficient of precision is closely related to the threshold of difference, a fact which would justify Gauss in putting this quantity directly proportional to the accuracy of observation. The threshold of difference is an object of psychophysical investigation, and for this reason I believe that the theory of physical measurement ought to be based on that of psychophysical measurement.

We now turn to Professor Safford's second objection. His criticism of my use of Lagrange's formula of interpolation is twofold: first that the calculation is carried entirely too far, and second that the interpolation should have been effected by the graphic method. Before entering upon the discussion of these objections I want to say something about the general purpose of interpolation. It frequently happens that the values of a function are given for a certain number of values of the independent variable, and that one wants to know something about the values of the function for intermediate values of the argument. Every procedure which serves this purpose is called a method of interpolation. In the graphic method one plots the results on millimeter paper and connects these points by a smooth curve. Every point of the curve corresponds to a certain value of the function, which in many cases may be read off with sufficient accuracy. This is the method which Professor Safford thinks I should have employed. I have done so as a matter of fact; but I did not consider such results of sufficient interest to publish, and only published a notice in the *Archiv f. d. ges. Psychologie*, 1910, Vol. 18, p. 410, that the charts are at the command of every scientific investigator, who may be interested in them. The chief objection against graphic interpolation is that it is too arbitrary; and for this reason I relied upon numerical interpolation alone.

There is a great variety of methods of numerical interpolation; but the essential feature of all these methods is that an algebraic expression is given, which may be fitted to the course of any function. Two of the best known methods are known as Newton's method of differences and Lagrange's formula of interpolation. Both methods are essentially identical, since they both suppose that the function may be represented by an algebraic function of degree n . The greater the number of observed values is, the more reliable is the result of interpolation; and it is, therefore, desirable to have as many observed values as possible. In a scientific investigation, however, one has to take into account that the time and energy of the observer are limited and that it is better to have a few carefully made observations than a mass of not very dependable results. In planning an investigation one has to strike a happy medium, which gives as many carefully made observations as possible. Experience shows that in the study of the psychometric functions seven values of the comparison stimulus are as much as can be handled easily and effectively. I may support in this respect my own opinion by the authority of G. E. Mueller. It is obvious that one cannot make observations for every intensity of the comparison stimulus, and that one has to fall back upon interpolation, if one wants to know something about the intermediate values. I, therefore, cannot see the reason why Professor Safford should find fault with my tables, because only seven entries were original results, a fact which could not fail to be noted by anybody who read the text. Tables of interpolated values are plentiful in physics and astronomy, and no one objects to them, if they are properly pointed out as such.

I now want to call attention to a small error in Professor Safford's text and a slight inconsistency in his position. He says on p. 97 that the seven ordinates were treated as absolutely exact. This is not quite correct; the abscissæ were so treated. He, furthermore, objects to the actual set-

ting up of the equations by Lagrange's formula on account of the great number of decimals which must be retained in the coefficients, but does not raise the same objection against the interpolation without setting up the equation. The advantage of this formula is that the interpolation can be effected without setting up the equation, but the use of the formula nevertheless implies the equation. To be consistent Professor Safford should have objected to the use of Lagrange's formula in any shape, but this would have precluded the use of Newton's formula, which Professor Safford favors, because it gives the same result "and requires about one-tenth of the labor." I beg to differ on this score. I tried both methods and found that the number of figures to be written down to effect the interpolation for one intermediate value and all my seven subjects was smaller for Lagrange's formula than for Newton's method of differences.

Professor Safford's clever criticism of the equation set up for the psychometric function for Subject *I* is likely to carry the most conviction to the reader. The discrepancy between the amount of work spent in setting up the equation and the result obtained is so great that one cannot possibly help being struck by it. In this part of the work I had the good fortune to obtain the services of a professional computer, who first set up the equation with six decimal places of the coefficients only. The results of interpolation by this formula were fantastic, because the curve did not follow the actual results at all. The necessity of retaining as many decimals as were actually used later on, began to dawn upon me only after I had reasoned out that each one of these coefficients had to be multiplied by high powers of numbers around 100. I then realized the necessity of carrying out all divisions to the bitter end, and incidentally won an insight into the nature of Lagrange's formula. This formula is a merely artificial construction, the coefficients of which have not immediate physical significance at all. The formula of interpolation is a means of achieving a certain purpose and he who wills the purpose must will the means.

It is very interesting to analyze Professor Safford's criticism of my statement that the use of Lagrange's formula does not imply a definite hypothesis about the psychometric functions. The meaning of this expression, which I explained at some length elsewhere, is this. The psychometric functions give the dependence of the probabilities of the different judgments on the intensity of the comparison stimulus. We do not know anything about this dependence, but we have to make some hypothesis about it for the purpose of interpolation. This can be done in two ways: by assuming a function which fits any kind of results, or by assuming a definite law of distribution. Lagrange's formula belongs to the first class, because the degree of the function depends on the number of observations only. The form of the function is, therefore, different in different cases. A further difference between these assumptions and a definite hypothesis about the psychometric functions consists in the fact that the latter admits of an extrapolation, whereas the former as a rule, do not.

I may illustrate this distinction by the following example. Suppose that a table be given, which we know contains the values of either the sine or tangent for small angles but we do not know which. We may use Lagrange's formula for interpolating in this table, and we may represent the course of the function in this interval by this formula, but we fully realize that this hypothesis is not definitive but subject to correction and that the formula will not represent the course of the function outside the interval. If, however, we possess some further information, which leads us to believe that the tables contain the values of the function sine, we make a definitive hypothesis about the function. My monograph on statistical methods contains only a casual mention of this distinction, but the articles in the *Archiv* leave no doubt as to the meaning which I wanted to convey by these words.

Professor Safford's criticism is this: "Lagrange's formula gives the equation of a curve through n points, whose degree is not greater than n (this is not correct, it should read $n-1$; remark of the writer), and the n points determine the curve completely." There is an infinity of curves of the same type and Lagrange's formula merely has the merit of being the simplest, and it is therefore useless to spend much energy upon it. This is the standpoint of the mathematician, whose interest lies in the study of the properties of whole groups of curves. The standpoint of the practical calculator is different. He attempts to reach his goal by the shortest possible route; and I, for one, refuse to consider a method of which I know beforehand that it has no merit over another excepting that it is more complicated. Whether in a scientific investigation energy is spent uselessly or not, depends upon the importance of the results obtained; and this can be judged by the specialist alone. I do not believe that my energy was wasted in this case, for I am willing to go through all the trouble of working out my data merely for the sake of finding the result, that the maximum of the psychometric function of the equality judgments must be related to the threshold of difference.

Professor Safford sees a further objection to the use of Lagrange's formula in the fact that it excludes at once all probability curves, symmetrical as well as asymmetrical. In my opinion, this is one of the greatest advantages of direct interpolation that it fits the actual data of observation without making the assumption of a definite law of distribution. If the data follow one of these functions, the interpolation by Lagrange's formula will follow it closely enough. I may remark that I am at this point in perfect agreement with W. Wirth, who in his latest publication employs the method of direct interpolation. One of the most distressing features of the history of psychophysics is the endless discussion as to the applicability of a definite law of distribution. It was my purpose to get away from this discussion and to see how the curves would look, if the data were not adjusted according to a definite law of distribution.

Professor Safford's last objection is based on the fact that the results of direct interpolation do not agree very well with what he calls the theoretical curves. I may remark here, that I made it a point to speak of hypothetical curves and not of theoretical curves. There is, of course, only little difference between a theory and an hypothesis, because many theories should be called hypotheses; but it seems to be a pretty general rule that one does not call a doctrine an hypothesis, unless one wants to emphasize the hypothetical element in it. The supposition that the psychometric functions belong to a certain type has the character of an hypothesis in a very high degree. There is no possibility of deciding beforehand whether the data will fit such a curve; and for this reason one ought to insist on calling such an assumption an hypothesis and not a theory.¹ The fact that the actual distribution does not coincide with the hypothetical one, is very

¹ I may be allowed to state here the reason which prompted me to choose the term $\phi(\gamma)$ hypothesis instead of the customary name of Gaussian distribution. It seems to me that this term should be restricted to the distribution of errors of observations exclusively. In the work of Gauss there is no passage known, to me, which could lead us to believe that Gauss would have applied this law to empirical distributions of all descriptions. Considering the practical turn of Gauss's mind, it seems very unlikely indeed that he would have favored such an unwarranted generalization. One, therefore, ought to speak of a distribution according to the probability integral or to the $\phi(\gamma)$ function. To call this function by the name of Kramp-Laplace, as Opitz has done, makes the name a little clumsy, and is not entirely justified from the historical point of view. It is entirely inadmissible to call this function by the name of Gauss; and the chances are that Gauss himself would have been very much surprised by this honor. In the *Theoria Motus Corporum Coelestium* (Werke, Vol. vii, p. 238) the integral is referred to Laplace, but as a matter of fact this integral was already known to Euler. Gauss was acquainted with this fact, as may be seen from a letter dated February 10, 1810, and from a manuscript note to the *Theoria Motus*. To call the function by the name of its inventor, one has to take one's stand in a complicated historical question, for the decision of which the complete material is not yet at hand.

interesting in view of the above mentioned discussion; but it is not so much an argument against the use of direct interpolation as one against the use of some hypothetical probability curve.

After paying me some compliments as to the mathematical treatment of my problems, of which, as coming from an authoritative source, I am highly appreciative, Professor Safford remarks that my data are hardly sufficient to warrant an extensive treatment. Of course I agree that it would be eminently desirable to have a more extended material, and that it would be possible to improve upon my results; but I must insist that it is at present the most suitable material for testing the different psychophysical methods. Other statistical sciences make very extended use of mathematical methods, and their material is not always as good as that of my experiments. The work of constructing mortality tables requires efforts compared to which my work—as being done by one individual—shrinks into insignificance; but the data used do not always show the high degree of stability of those found in my experiments. This fact may readily be seen from the values of the coefficients of divergence: But few statistical investigations deal with data of a similar degree of stability. I do hope that we may soon possess an experimental material still more extended than my own; but until then my data are the best available for the specific purpose of testing the different psychophysical methods. And if one wants to make such a test one has to make the best of the material at hand.

Professor Safford has presented his criticism with admirable clearness and precision; but I am convinced that his position is untenable, and his argument is unsound. The methods of physical measurement cannot be taken over bodily and applied directly to the problems of statistics and of psychophysics. And while we are always grateful for the mathematician's interest in psychophysical discussions, yet it is a truism that every science is obliged to develop its own methods, and to grapple with its problems in its own way. That the methods and the problems of the theory of observations and of psychophysics are the same cannot be maintained; whether, indeed, any intimate relationship obtains between these two fields of scientific endeavor still remains to be determined.